in others with doubtful results. This is, however, only what might be expected in view of the diversified difficulties which must necessarily arise.

There has been a veritable flood of malaria literature of recent years, including an annual volume of "Atti della Società per gli studi della malaria," the series commencing in 1900, which has come to us from Italy. Mosquitoes have received an immense amount of attention, after being much neglected in the past. The number of genera and species and their classification have become subjects to bewilder all but specialists.

The important discoveries on sleeping sickness ushered in by the researches of Castellani, a pupil of this school, have been confirmed and extended by Bruce and his collaborators of the sleeping sickness commission. The relation of the flies belonging to the genus Glossina to the transmission of the trypanosomes is being actively studied, and many important questions we must hope are nearing their solution in connection with this most fatal malady. A contribution has just come to hand from Gray and Tulloch with regard to the multiplication of the parasites in Glossina, indicating that the belief recently expressed is warranted, namely, that the parasites undergo a cycle of development within the insects. Of importance in their bearing on the question of the development of trypanosomes in other than their vertebrate hosts are the investigations of Schaudinn on *Trypanosoma noctuae* in Culex, those of Brumpt on certain trypanosomes of fishes which undergo their cycle of development in leeches, and those of Prowazeck on the rat trypanosome, which he has demonstrated undergoes development in the rat louse (*Haematopinus spinulosus*).

Of recent discoveries, the one which to me appears to carry the greatest weight is that of Novy and McNeal. They have been the first to obtain pure cultures of Protozoa, maintaining trypanosomes of different species alive *in vitro* for many generations. There is no telling whither the methods they have given us may lead; they directly stimulated Leonard Rogers to experiments wherein he succeeded, by an ingenious method of his own, in cultivating another protozoon, the Leishmania, obtained from cases of kala-azar.

The work on the tick-transmitted diseases known as the piroplasmoses (redwater, &c.) occurring in cattle, sheep, horses, and dogs has been pursued in various parts of the world with great activity. The results appear to me to indicate, what I believe also holds for human malaria parasites, that we shall in time learn to distinguish different parasites which we at present consider to represent single species.

The investigations of Dutton and Todd on tick fever in the Congo Free State, announced in February, have gone to prove that this disease is transmitted by a tick (*Ornithodoros savignyi*) after it has infected itself with blood containing the Spirochætæ. This has been confirmed by Koch, if we may rely on what has appeared recently in German newspapers. It is, however, quite premature to assume that African tick fever and European relapsing fever are due to one and the same species of Spirochæta; in fact, it is highly probable that this is not the case, although the report in question refers to the Spirochætæ as one species. In relapsing fever in Europe the bedbug (Cimex) has long been suspected to be a carrier of the infective agent, a probability which was considerably heightened by Karlinski's observation of motile Spirochætæ in the bodies of the insects up to thirty days after they had fed on relapsing fever blood. Schaudinn, moreover, informs me that he has observed the multiplication of the *Spirochaeta obermeieri* in Cimex. These observations, following closely upon those published by Marchoux and Salimbeni, are of greatest interest and practical import. The last named authors demonstrated that a fatal disease of the fowl in Brazil is due to a Spirochæta which is transmitted through the agency of a tick (*Argas miniatus*), and this is capable of conveying the disease even six months after feeding on infected blood. These Spirochætæ multiply in the tick, and are present in large quantities in its body cavity throughout this period. These observations are very suggestive, since they demonstrate the long persistence of the parasites in their carriers, and render it probable that they will be found to be harboured much longer. Finally, the finding this year of *Spirochaeta pallida* in syphilis by Schaudinn and others in man, and by Metschnikoff and Roux in experimentally infected apes, cannot escape a passing notice.

It is of some interest to note that the close blood-relationship existing between the apes and man, demonstrated independently by means of the precipitins by Grünbaum and myself, served as a direct incentive for the experiments of Metschnikoff and Roux, Lassar, and Neisser, which proved that human syphilis is communicable to the chimpanzee and ourang outang.

Of interest has also been the further discovery this year of a number of new protozoal parasites in the blood of different animals, in addition to numerous new species of Trypanosoma. I refer to new forms called "Leucocytozoa" because they inhabit the white blood corpuscles of their vertebrate hosts. Leucocytozoa were first discovered by Bentley in dogs in India, and were described, without sufficient mention of this fact, by James. Another species has been found by A. Balfour in the rat (*M. decumanus*) in Khartoum; and lastly, W. S. Patton informs me that he has found a species in the squirrel (*Sciurus palmarum*) in India, and apparently observed developmental forms thereof in a louse. Balfour has, moreover, described a new Hæmogregarine occurring in the jerboa (*Jaculus jaculus*), and Graham-Smith in our laboratory has found another new endoglobular parasite in the mole. This by no means exhausts the "finds" of this year, but it will suffice to show that British workers are doing their share in furthering our knowledge in this regard.

Of the diseases due to Vermes I can say but little. The discovery of Catto's Schisostomum in this laboratory is familiar to you all. It is interesting to note, following on the experiments with *Ankylostoma duodenale* by Loos, proving that the embryo worm can infect by penetrating through the skin, that Boycott in London and Tenholt in Germany have confirmed the fact this year in two experiments conducted on medical men who volunteered for the purpose.

Again, it is apparent that the subject of immunity in relation to protozoal diseases is proving to be one of great difficulty, and the results hitherto obtained indicate that new methods will have to be devised if the problem is to be solved from a practical, and still more so from a scientific, standpoint. It is also obvious in this connection that the problems before us can only be solved by animal experiment, and this accentuates the need of our giving an increasing amount of attention to comparative pathology as we push on toward the alleviation of the ills to which our own flesh is heir.

Many matters have necessarily been left untouched, including even such important diseases as yellow fever and Malta fever, on which active work has been done. My object has been to seize upon a few salient facts with the view of showing how much has been accomplished within a short period, and how great are the opportunities of the workers in this school who are destined to labour in new fields in different parts of the world. Perhaps what I have said—in no spirit of presumption—will serve as an incentive to some of my hearers. Let me conclude with some wise words from the Talmud:—

> "The day is short and the work is great.
> It is not incumbent upon thee to complete
> the work, but thou must not therefore
> cease from it."

THE MECHANICS OF THE ASCENT OF SAP IN TREES.[1]

THE following remarks, relating to one of the most powerful and universal of the mechanical operations of organic nature, are based mainly on the numerous experimental results reported in Dr. A. J. Ewart's recent memoir.[2] Their chief object is to assert the view that we are not compelled to suppose the sap, in the column of vessels through which it rises, to be subject to the great actual pressure, amounting in high trees to many atmospheres, that is sometimes postulated. It is hardly

---

[1] By Prof. J. Larmor, Sec R.S. Paper received at the Royal Society on June 29.

[2] *Roy. Soc. Proc.*, vol. lxxiv. p. 554; *Phil. Trans.*, B, vol. cxcviii. p. 41.

© 1905 Nature Publishing Group

necessary to remark that the problem of the rise of sap is one of mechanics, in so far as concerns the mode of the flow and the propelling power.

Contrary to the view above referred to, it seems not unreasonable to consider that the weight of the sap in each vessel is sustained in the main by the walls and base of that vessel, instead of being transmitted through its osmotically porous base to the vessels beneath it, and thus accumulated as hydrostatic pressure.

We could in fact imagine, diagrammatically (as happens in ordinary osmotic arrangements), a vertical column of vessels, each provided, say, with a short vertical side-tube communicating with the open air, in which the pressure is adjusted from moment to moment, and yet such that the sap slowly travels by transpiration from each vessel to the one next above, through the porous partitions between them, provided there is an upward osmotic gradient, *i.e.* if the dissolved substances are maintained in greater concentration in the higher vessels.[1] This difference of density must be great enough, between adjacent vessels, to introduce osmotic pressure in excess of that required to balance the head of fluid in the length of the upper one, into which the water has to force its way. Thus, in comparing vessels at different levels, the sap must be more concentrated in the upper ones by amounts corresponding to osmotic pressure more than counteracting the total head due to difference of levels, in order that it may be able to rise. As osmotic pressure is comparable with gaseous pressure for the same density of the molecules of the dissolved substance, the concentration required on this view is considerable, though not very great.

Such a steady gradient of concentration could apparently, on the whole, become self-adjusting, through assistance from the vital stimuli of the plant, for concentration in the upper vessels is promoted by evaporation. Yet pressures in excess or defect of the normal atmospheric amount might at times accumulate locally, the latter giving rise to the bubbles observed in the vessels, through release of dissolved gases.

It may be that this assumes too much concentration of dissolved material in the sap, *as it exists inside the vessels of the stem,* to agree with fact. In that case the capillary suction exerted from the nearest leaf-surface might be brought into requisition, after the manner of Dixon and Joly, to assist in drawing off the excess of water from the vessels. The aim proposed in this note is not to explain how things happen, which is a matter for observation and experiment, but merely to support the position that nothing abnormal from the passive mechanical point of view need be involved in this or other vital phenomena.

As regards estimating the amount of flow, at first sight it may not appear obvious, *a priori,* that the transpiration through a porous partition or membrane, due to osmotic gradient, is equal or even comparable in amount to what would be produced, with pure water, by a hydrostatic pressure-head equal to the difference of the osmotic pressures on the two faces of the partition. But more exact consideration shows that, on the contrary, osmotic pressure is *defined* by this very equality;[2] it is that pressure-difference which would produce such an opposite percolation of water as would just balance the direct percolation due to the osmotic attraction of the salt-solution.

It would, however, appear that the great resistance to flow offered by what botanists call Jamin-tubes,. viz. thin liquid columns containing and carrying along numerous broad air-bubbles, is conditioned mainly by the viscosity of the fluid, and involves only indirectly the surface-tension of the bubbles. In fact, the resistance to flow may be expected to remain much the same if each bubble were replaced by a flat solid disc, nearly but not quite fitting the tube. Its high value arises from the circumstance that the mass of liquid between two discs moves on nearly as a solid block when the flow is steady, so that the viscous sliding has to take place in a thin layer close to the wall of the tube, and is on that account the more intense, and the friction against the tube the greater. The increased curvature of the upper capillary meniscus of the bubble is thus merely a gauge of the greater intensity of the viscous resistance instead of its cause, and modification of the surface-tension cannot be involved as a propelling power. The experimental numbers given by Dr. Ewart show that, even where the vessels are largely occupied by bubbles, the greater part of the resistance to active transpiration still resides in the partitions between them.

If the osmotic gradient, assisted possibly by capillary pull at the leaf-orifices, is insufficient to direct a current of transpiration upward, *capillary* alterations inside the vessels, arising from vitally controlled emission and absorption of material from the walls, cannot be invoked to assist: rather it must be *osmotic* alterations from one vessel to the next, of, so to speak, a peristaltic character, that might thus come into play. But any such alteration (of either kind) will involve local supply of energy. Is there a sufficient fund of energy, latent in the stem, to provide permanently the motive power for the elevation of the sap? In what form could this energy get transported there? The energies of the plant-economy come from the sunlight absorbed by the leaves. The natural view would appear to be that the work required to lift the sap is exerted at the place where the energy is received, and that it operates through extrusion of water by evaporative processes working against the osmotic attraction of the dissolved salts; while the maintenance of equilibrium along the vessels of the balanced osmotic column, with its semi-permeable partitions, demands that an equal amount of water must rise spontaneously to take the place of what is thus removed.

The subject might, perhaps, be further elucidated by observation of the manner in which the flow is first established at the beginning of the season, or possibly by experiments on the rate at which water would be absorbed by a wounded stem high above the ground.

---

[1] Thus, in an ordinary osmotic experiment with a **U**-tube, the percolation of water through the plug gradually *produces* a difference of hydrostatic pressure on its two faces, which is *sustained* by the fixity of the plug itself, but would be at once neutralised if the plug were free to slide in the tube. This increase of volume of the salt-solution, by the percolation of pure water into it, is on the van 't Hoff analogy correlated with the free expansion of the molecules constituting a gas. It goes on with diminished speed under opposing pressure, until a definite neutralising pressure is reached, inaptly called the osmotic pressure of the molecules of the solute, which just stops it, while higher pressures would reverse it. The stoppage is due to the establishment of a balance between the amounts of water percolating one way under osmotic attraction, and the opposite way under hydrostatic pressure. The pressure established, *e.g.* in an organic cell immersed in salt-solution, is thus really the reaction which is set up against the osmotic process. That process itself is perhaps more directly and intelligibly described as the play of osmotic affinity or attraction, even though it must be counted as of the same nature as the affinity of a gas for a vacuum. *Cf. Proc. Camb. Phil. Soc.*, January, 1897, or Whetham's "Theory of Solution," p. 109.

[2] See preceding footnote.

EXPERIMENTS WITH THE LANGLEY AËRODROME.[1]

THE experiments undertaken by the Smithsonian Institution upon an aërodrome, or flying machine, capable of carrying a man have been suspended from lack of funds to repair defects in the launching apparatus without the machine ever having been in the air at all. As these experiments have been popularly, and of late repeatedly, represented as having failed on the contrary, because the aërodrome could not sustain itself in the air, I have decided to give this brief though late account, which may be accepted as the first authoritative statement of them.

It will be remembered that in 1896 wholly successful flights of between one-half and one mile by large steam-driven models, unsupported except by the mechanical effects of steam engines, had been made by me. In all these the machine was first launched into the air from "ways," somewhat as a ship is launched into the water, the machine resting on a car that ran forward on these ways, which fell down at the extremity of the car's motion, releasing the aërodrome for its free flight.

In the early part of 1898 the Board of Ordnance and Fortification of the War Department allotted 50,000 dollars for the development, construction, and test of a large aëro-

---

[1] Abridged from a paper by Dr. S. P. Langley in the Smithsonian Report for 1904.

© 1905 Nature Publishing Group